JOURNAL
OF THE
WASHINGTON ACADEMY OF SCIENCES

VOL. 16 FEBRUARY 4, 1926 No. 3

ENTOMOLOGY.—*Entomological taxonomy: its aims and failures.*[1]

1. FROM A TAXONOMIC VIEWPOINT. S. A. ROHWER, Bureau of Entomology

When the idea of this symposium occurred to the chairman of the Communication Committee, it is very probable that he had recently seen some paper of a "taxonomic" nature which seemed to be lacking in a number of desired features. Otherwise the symposium would probably have been given a different subtitle, for I doubt very much the propriety of the use of the word "failures." The strongest idea it was intended to convey was "shortcomings." Be that as it may, we have accepted the subject for discussion and I think it is one we may well discuss. The science of Biology has made remarkable strides in the last twenty years. It has had opened before it many lines of investigation which were heretofore unknown.

Some of these new studies have gained such popularity that their patrons have thought so well of themselves and the importance of their investigations that they have coined new "ologies" to separate themselves from the other workers. All this time taxonomy has continued and has attracted the attention of only a few. More recently, however, the pendulum has swung back and today the classifier is held in more esteem. The time seems to be passing when it will be necessary to apologize for the fact that one is a taxonomist. This returning into the good graces will not last long unless the students of taxonomy avail themselves of the materials which have been gathered by investigators in related fields, for taxonomy can not be a deaf and dumb science and still live. For this reason it seems desirable to discuss the aims of taxonomy, and as we consider these perhaps we may in our reflection see some shortcomings.

[1] Papers presented at the 373d meeting of the Entomological Society of Washington, held March 5, 1925.

Before getting too far into the subject it will be well to accept, at least for the moment, a definition of taxonomy; and while we may not all agree, I venture the following for consideration. Taxonomy consists of the grouping of organisms in a phylogenetic manner after a consideration of all of their characters and characteristics.

Accepting such a broad definition, the taxonomist must base his classification not only on external morphology but he must also call to his aid anatomy, physiology, embryology, cytology, ecology, paleontology, and distribution; in fact, he must consider his organism not by itself alone, but he must understand its function and its place in relation to other organisms past and present. To do all this is no small task, and to say that, is not all. If in entomology we were dealing with a limited number of forms and if these forms had such habits as to permit a detailed study of them, the task would be of sufficient magnitude. But when we consider that conservatively estimated there are about 640,920 described insects, and that this represents perhaps less than one-tenth of the forms which actually exist, and that for most of these 640,920 forms, we know only a few cabinet specimens of adults and nothing concerning their habits, the task becomes stupendous. It is very probable that this very fact has caused the taxonomist to become so deeply involved in the details that he has lost sight of other allied "ologies," and thus received such criticisms as "Oh! he is only a narrow taxonomist." But let us not stop with these apologies. We grant the magnitude of the task and we admit also that some very good results have apparently been obtained by a careful comparison of morphology. If good results have been accomplished by a study of parts, how much better the results will be if we consider the whole.

But let us go back and consider briefly some of the various lines of investigation a taxonomist should be familiar with and include in his consideration when making a phylogenetic grouping. I imagined I saw a shaking of the head when I suggested paleontology—I hope not. Yet most taxonomic entomologists ignore the fossils. So much are they forgotten that many times they are not cataloged. Such an attitude can not be defended by any scientific excuse. Where would be the classifications and the fundamental results derived from them in mammalogy had the fossils been thrown aside because there were too many recent things to describe?

When I used the word "anatomy" a short while ago I meant to restrict the use of the word somewhat, and had in mind more a consideration of the internal softer organs. So little is known concerning

these in insects that not much can be said, yet when more is known and their function better understood, I venture the suggestion that the taxonomist will find valuable evidence to refute or uphold his major groupings. Of embryology and cytology little can be said, yet both of these lines of investigation will furnish valuable aids to a true phylogenetic arrangement. Distribution if studied carefully will often prove of great aid. When I hear discussions of so-called discontinuous distribution, the first thought that comes to my mind is, how about the true relationships? Perhaps many of the examples of discontinuous distribution are due to faulty taxonomy. If there is anything in this thought then a study of distribution may help the taxonomist to see some of the weak points of his classification; hence it is a line of study the taxonomist should consider. And there is also the converse, for a study of distribution may just as well tend to show relationships.

In including ecology in the list of fields from which the taxonomist must expect aid, I have ventured to use a comprehensive definition of the word "ecology," and I have therefore included under this head the information usually listed by taxonomists under such headings as "host," "habitat," and "habits." Taxonomists have long paid considerable attention to the host and host plants, and to a lesser extent have they considered the habitat and habits. The consideration of these points is of importance, and when we get the phylogenetic point of view it becomes more so. We cannot logically expect that groups which have complex host relationships and specialized habits will give rise to groups with simple host relationships and generalized habits. Such may be the case. Cases of reversion are known, but a classification which indicated that this was true might well be carefully and critically examined before it is pronounced as having been made along phylogenetic lines.

We have considered only very briefly some of the points but before my time is completely gone, I want to include a word about nomenclature, the bug-bear of most taxonomists. I said "most" and I believe advisedly because there are some who have in my opinion so completely forgotten the true significance of nomenclature as to be in the position of trying to put the cart before the horse. My appreciation for the standardization of names, the application of general rules and suggestions on procedure is very great. In fact I fully appreciate nomenclature, so much so that I have been guilty of doing nomenclatorial things. But I have not as yet forgotten, and I trust I never shall forget, that nomenclature, as we entomologists use the word, is only a

handmaid to zoology. Nomenclature deals with names, not animals. I venture the guess that less than ten per cent of the changes in the names of insects are due to nomenclature. Most of them are due to a change in the conceptions of groups. In other words, they are made for zoological reasons. It must be so. The classification of insects must change. New facts are before us every day. We apply these in our taxonomic work and we change the name of some little insect or other. Such a change is not due to nomenclature. But I have almost forgotten why I brought up this handmaid to taxonomy. No taxonomist likes to change names but no taxonomic work, however sound from a phylogenetic point of view, can stand for a long period of usefulness unless its author carefully considers the nomenclature of the group. It is essential that entomologists agree on names, and if all taxonomic workers hasten to establish the landmarks by which group names may be recognized, fewer changes will be necessary and their work will be of a more permanent nature. The establishment of genotypes for all genera and especially those on which supergeneric names are founded is important, and to a very large extent this must be done by the taxonomist.

In our definition we said taxonomy was the grouping of organisms and this presupposes there are organisms to be grouped. So a study of taxonomy must first await the accumulation of materials. A taxonomist without a collection is as bad off as the man at sea without water and the one with a small collection is perhaps, as far as real progress is concerned, worse off. If proper taxonomic work can be done only when all factors are considered then to work in a taxonomic way over only an incomplete assemblage of specimens can not produce good results. In almost every group in insects we have examples of poorly constructed classifications because of an examination of an inadequate number of specimens. We must not discourage the collecting instinct in the taxonomist. On the other hand we should lend him all encouragement. We should place at his disposal for study all the material of his group. He should have material from all regions and in sufficient abundance for him to study the variation of individuals.

This need for collections imposes an obligation on the taxonomist as well as those who foster his work. It makes it necessary for him to care for these collections; they must be arranged in a careful, orderly manner; they must be labelled. The taxonomist must leave to his science and posterity evidence from which he made his conclusions. There must be no doubt about the fact that certain specimens were seen. The taxonomist has therefore devised a method by which his

co-workers and successors can know what he was talking about. He calls certain specimens types. But this is not enough; he forms conceptions about other workers' groups and he must leave evidences of the limits of his conception. Here many workers are negligent. They do not tell us definitely about these. The aim of all taxonomists should be to leave the evidences of their work in such good order as to leave no doubt in the minds of other workers on what their conclusions were founded. In short the taxonomist should care for his collections and arrange and label them so as to aid, not hinder, other investigators. I am sure all of you could cite many shortcomings here.

Another aim of taxonomists is large libraries. The taxonomist must know what others have done. In a field as vast as entomology this is of the greatest importance. It is impossible for one worker to know all. It is imperative that he know what has been done before. Large libraries must also be considered a necessary aid to taxonomic work. But libraries are of but little use unless one knows what is in them and where to find it, so indices are necessary. In view of the rapidity with which work is being published, these indices must be up to date to be of real service. While in a certain sense one can hardly say these libraries and indices are aims to taxonomic entomology, we must admit they are aims of taxonomic entomologists, and you all will agree it would fill a large volume to list the shortcomings because of their lack.

Summing up briefly, the aim of taxonomic entomology should be the phylogenetic classification of insects based on all available evidence, such evidence to include a consideration of anatomy, morphology, embryology, cytology, physiology, paleontology, ecology and distribution. If such are the aims of taxonomy then we have only to examine our literature to see how completely we have met them. Such a consideration of the literature would probably make many feel that there had been many shortcomings. Of course there have. But many of them are due to the magnitude of the task and some of them are due to the changing viewpoint.

I hope the viewpoint may continue to change, that taxonomists will continue to include in their papers more and more information concerning all the characters and characteristics of the insects they treat. Many taxonomists have much of this information at their command and use it consciously or unconsciously in forming their classifications. Let us urge them to include more of it in their papers so they may be storehouses of information to other workers. By doing so their usefulness will greatly increase and they will rise in the esteem of workers in allied fields.

2. From an Economic Viewpoint. A. C. Baker, Bureau of Entomology

I have been asked to discuss the relation of taxonomic entomology to economic entomology and the failures of the former in this relationship. Such a request in itself indicates the failure I shall mention. Perhaps, however, it is not a failure. Perhaps it is merely a circumstance incident to growth.

Lest I be misunderstood I wish to distinguish clearly between the science of insect life and those practices in the art of agriculture which concern themselves with insects and with which entomologists as agricultural advisors have much to do. This dual function of the entomologist, as advisor and as discoverer, has confused certain practices of the art with the science that underlies them. I presume that I am not expected to discuss the relation of taxonomic entomology to the art of agriculture.

Since this symposium is on taxonomy it may be well at the outset to delimit the different fields that are often confused with taxonomy by reason of the fact that taxonomists work in them. We must distinguish taxonomy, classification, and nomenclature. Taxonomy, as its name implies, is not concerned with the arrangement as such but with the reasons and causes back of that arrangement, with the underlying principles. Classification, on the other hand, constitutes the arrangement itself. Thus the same taxonomy may be employed in a classification of a family of Hemiptera or in that of a family of Hymenoptera. Nomenclature, again, is a subject which is concerned with the correct names for the units in a classification. It deals neither with the methods back of the classification nor with the classification itself. Thus we have nomenclature as a result of classification and classification as a result of taxonomy. In this relationship taxonomy is basic.

As I see it, there are three types of taxonomic entomology today, and these three types recapitulate the three stages in its growth.

The first is the accumulative type. Here the main interest centers on the collection. The aim is to complete the series, to amass material. Species are described. These are carefully placed away, perhaps according to some accepted classification, and other species are described. Of this type I shall have little to say for the reason that it concerns itself very little with taxonomy as I understand it. In many cases even the classification is already a fixed conception. The author merely adds to the nomenclature of that classification in the naming

of species not already included. In regard to this type, however, I shall say one thing. It might be of enormous advantage. As it stands today its devotees are interested in individual groups. They pick these from the population and ignore the others. But in a study of the accumulative type the interest should lie in the equilibrium of the population. It is better to know the workings of a field than the disconnected items of a world.

The second type is the morphological one. Here the main interest centers on structure. Dissection is not uncommon and an attempt is made to reconstruct the relationships by means of the structures studied. Phylogenetic trees therefore are the mode and theoretical discussions are common. There may even develop a voluminous literature on the interpretation that should be placed on the veins of the wings or the spines of the legs. Most taxonomic entomology today is of this type. Perhaps it is so of necessity. While I realize the valuable contributions that have been made from this viewpoint and the great handicaps under which brilliant men have labored in this field, I can not help feeling that this type of taxonomy has one decided fault. The structure is the primary concept and in concentration upon it the entomologist is apt to lose sight of his real goal. The broader visioned taxonomists of the morphological school, however, are alive to this danger. Hence they constantly discuss and write about the suitability of characters. They talk of natural characters and of artificial characters, but they do not tell us how one character can be more natural or more artificial than another.

The third type is the biotic one. Here the main interest centers on the insect alive rather than on its dead body. The taxonomic laboratory is no longer an orderly array of dead insects. It is a dynamic world of living things. In its fullest realization this type requires some departure from the usually accepted ideas. Side by side with the collection will be, not only the morphological laboratory, but the insectary where the insects may be studied alive. And beyond all this there will be the outdoors. The taxonomist will once again become the naturalist, but with this difference he will have at his command a great store of modern technical methods.

The biotic type of taxonomy will not only change the work, the publications too will change. They will be appreciated. A monograph of a genus will no longer lie uncut upon the shelf. It will become a live book full of interest for the biologist, the agriculturist and the physician. It will be used and its author will receive the credit he deserves.

TABLE 1.—Classification of Subfamily Eriosomatinae

Forming galls or pseudo-galls on trees and migrating to trees or herbs, usually to the roots

GENUS	PRIMARY PHASE	SECONDARY PHASE
Elm Association—Tribe Eriosomatini		
Eriosoma	Elm galls or pseudo-galls	Roots of woody plants
Colopha	Elm galls	Roots of grasses
Georgia	Elm galls	?
Tetraneura	Elm galls	Roots of grasses
Dryopeia	Elm galls	Roots of grasses
Gobiashia	Elm galls	?
Poplar Association—Tribe Pemphigini		
Pemphigus	Poplar galls	Roots of herbs
Cornaphis	Poplar galls	?
Mordwilkoja	Poplar galls	?
Pachypappella	Poplar galls	?
Pachypappa	Poplar galls	?
Asiphum	Poplar pseudo-galls	?
Thecabius	Poplar galls	Herbs
Gootiella	Poplar galls	?
Pistacia Association—Tribe Fordini		
Forda	Galls on Pistacia	Roots and nests of ants
Aploneura	Galls on Pistacia	?
Pemphigella	Galls on Pistacia	Roots and nests of ants
Paracletus	Galls on Pistacia	Nests of ants
Geoica	?	Roots attended by ants
Tullgrenia	?	Roots attended by ants
Trifidaphis	?	Roots attended by ants
Damp woods Association—Tribe Prociphilini		
Neoprociphilus	Maple	Smilax
Patchiella	Tilia, Ash	?
Prociphilus	Ash	Roots of plants
	Maple	Alder stems
	Amelanchier	
	Crataegus	
	Lonicera	
Rhus Association—Tribe Melaphini		
Melaphis	Galls on Rhus	Roots of moss
Nurudea	Galls on Rhus	?

Most work today is associated with the evolutionary viewpoint. As taxonomists, however, we have conceived of morphologic evolution. We have concentrated upon supposed species. But if there is an evolution it is the entire environmental complex that evolves. Things change only in relation to other things. Perhaps I can make myself clear by saying that taxonomy should concern itself with events more, with supposed things less, with the quantitative record of conditions all the time. Our enthronement of type specimens is an admission of the failure of our taxonomic method.

I may be pardoned if I refer to the group on which I have worked the most, the aphids. My excuse is that I know this group the best. Five years ago I presented a classification of this family. That classification was woefully inadequate. In order to illustrate the taxonomy employed, however, I am showing a tabulation of one subfamily, the Eriosomatinae (Table 1). It will be noted that an attempt was first made to determine something of the living insects. Host relation was selected by reason of the fact that the insects are peculiarly phytophagus. The selection thus of one factor is admittedly weak. For as it is, the total association evolves so that it is the assemblage of factors that must picture the events. One factor however appears at times to be almost a master one and to reflect the others. On this possibility we have chosen host relation in this subfamily. The primary phase of the life cycle was accepted as fundamental for reasons that are obvious.

It will be noted that certain associations at once become evident, such as the Elm Association, the Poplar Association, and the Pistacia Association. The insects falling in these associations were again segregated, using type species and the habits of type species as a basis. The list of genera falling in the Elm Association reveals certain morphological characters common to all species and peculiar to the genera in this Association. These characters therefore distinguish the tribe. A similar examination of the forms in the Poplar Association shows other characters peculiar to these genera and common to them. The correct diagnosis of the tribe Pemphigini therefore becomes evident. And so the examination proceeds throughout all of the associations. In the end we have tribal descriptions which reflect not only structures common to the insects falling therein, but life habits which are equally common to them—a classification of the animals alive.

It will be urged by some that taxonomic studies of this kind deal altogether with secondary things, that structure is basic. But if we

accept evolution surely it is activity that is basic. Unrelated forms may of course show similar habits but such forms would segregate earlier on other biotic factors.

But aside from this question the economic value of the taxonomy employed will be clear if we look for a moment at the Tribe Fordini. Species of the genus *Forda* are common in this country on the roots of plants and in ants' nests. Considerable study has been given to the species, and occasional revisions or partial revisions have been published. But these revisions left us in much the same state as we were before, for the reason that the investigators worked from the morphological viewpoint. More supposed species were described, but this only meant, at bottom, a more complete catalogue of our ignorance, for the work was all done on the incomplete secondary phases of the life cycles. The workers did not conceive of the Pistacia Association. Had they done so they would have realized that the key to the genus on this continent lay only in Texas and southward, and that years might be spent on the secondary northern remnants of these Pistacia forms without any real advance in knowledge.

A similar picture of this very kind is the history of the study of the woolly apple aphis, *Erisoma lanigerum*. For a hundred years men tried to solve the life history of this economic insect. Medals and prizes were offered for its solution. Years of research and large sums of money were spent without result.

A glance at the biotic arrangement on the screen will show how simple the solution becomes; and it is equally simple in other instances. When we find another species of *Eriosoma* as a pest on pear roots we turn at once to the elms. When we find still another very injurious to the roots of gooseberries we turn once more to the elms. Still another species is abundant on the roots of service berry and once again we take our way to the elms.

Another example may be given. When a *Pemphigus* is discovered as a pest of the beet fields we can turn at once to the poplars for its complete cycle. In another region the poplar segregated does not exist but the beets are nevertheless attacked. So we find a different poplar with a different *Pemphigus* migrating to the beets as before. Still another species is a pest of crucifers, and turning to the poplars we can determine its identity and the economic factors involved.

Time will not permit me to follow the argument further, but I shall give one word in regard to the reception this work has had. My paper in 1920 did not give completely my taxonomy. For obvious

reasons I contented myself with a classification—with tabulating and discussing the characters resulting from the taxonomic study. Nevertheless a thorough student might discover the method in the background. Such a student is Professor Albert Tullgren of Sweden. In 1925 he referred to my classification in the following words:

"One of the most important and in parts most interesting systematic work on aphids that has been published in the last ten years is A. C. Baker's Generic Classification of the Hemipterous family Aphididae. Baker presents, often in a very alluring manner an entirely new system for the Aphididae and bases it on reasoning which often has a very convincing effect. He divides the entire family into 4 sub-families, Aphidinae, Mindarinae, Eriosomatinae and Hormaphidinae which are among themselves almost equal although the Aphidinae and the other three subfamilies are derived from two different origins of the hypothetical stem. The reasons given for this separation into 4 subfamilies do not appear to me, however, to be entirely free of criticism and I deem it therefore more cautious for the present to consider the three last groups as one subfamily."

And again he says:

"Baker divided his subfamily Eriosomatinae into five different groups, Eriosomatini, Pemphigini, Malaphini, Prociphilini and Fordini. If one studies closely the characteristics of differentiation one finds that he derived the same first of all from the biological differences of the generic elements. And one can not help thinking that he put a higher value on these characteristics than on the morphological ones. For this reason presumably he has arrived at the peculiar conclusion, according to my opinion, that the Pemphigini and the Prociphilini represent two different branches of the stem which are about equal to the Eriosomatini."

I have cited Tullgren because I know him to be a scholar. Perhaps he is right. I forsee the day, however, when the taxonomist will not be set apart from the economic entomologist, when the collector will concentrate on true samples of the population, when the morphologist will consider function as important as form, and when all life history studies will be made by taxonomists of the biotic school. When that day comes there will be only one type of entomology. It will be economic. Its aim will be to understand and to express with mathematical exactness the laws and principles underlying the elements, the contacts and the inter-relations of the insect world. We are fast approaching the saturation point of our population and the day may not be far distant when we shall be pressed for that understanding.

3. From an Educational Viewpoint. E. D. Ball, Department of Agriculture

Taxonomy in its highest development, as I conceive it, is an explanation of the actual relationship of existing forms of life to each other. Although of necessity expressed in a linear series it should be an arrangement of the existing branches of the tree of life into groups according to their derivation and into a series showing inter-relationship of the groups. In the major branches of both botany and zoology, taxonomy has already approached this idea. When it comes to the lesser divisions and more obscure relationships it is still far from certain of its foundations and is undergoing a gradual evolution as new discoveries in fossil forms are made and new interpretations of relationships in living species are established. Taxonomy, then, in its ideals is an interpretation of evolution, one of the most profoundly interesting and profitable fields of biological research.

Taxonomy in its lowest expression is merely an enumeration of a group of individuals. Enumerating individuals for taxing purposes was man's earliest effort and from this the science received its name. Some taxonomy has not materially advanced above this level. Let us illustrate: It would be possible to classify an indefinite number of wooden blocks of different shapes so that each one of a given group would fall into a definite category. The primary division might easily be (A) long blocks; (AA) short blocks; and (B and BB) under each one might be blocks with right angles and blocks without right angles, and so on indefinitely, and when you finished your task you would have a classification for taxing purposes only. It certainly would not be of value for any other purpose. You could take a saw and in a few minutes change a given block so that it would go into an entirely different classification. Your classification was therefore entirely artificial and empirical. On the other hand, you *might* have classified your blocks into hard woods and soft woods. You might have gone further and classified your soft woods with reference to certain structures which would have separated the coniferous from the deciduous forms, and continued this segregation to a completion of the group. Such a classification could not be altered by any use of a saw. The block of wood would fall into its correct classification regardless of what was done to it. In other words, it would have been a classification rather than an enumeration. In many of our taxonomic efforts, especially where working with a very small representation of a group or with little knowledge of ancestral forms, our classifications may be very

little better than the long and short sticks of wood, but if we attempt to make a rational classification and follow it as far as our knowledge at the moment permits, correcting it from time to time as our knowledge increases, we are doing the best we can and following the path of the evolution of all knowledge.

There existed for a long period a large school of morphologists who openly ignored and belittled taxonomy. Happily that day is passing. I remember working in a laboratory for a year with an earnest and conscientious young man who was working industriously tracing the development of the lateral line and its sense organs in an embryo of a salamander. I was at the same time working on the evolution (taxonomy if you please) of a certain group of leafhoppers and we used to have frequent arguments as to the value of taxonomy, a value he did not at that time recognize. When, however, he had his work completed and was preparing it for publication he suddenly discovered that there were other genera of salamanders and that the references which he had been consulting were all about a certain common species. Not knowing that there were other genera he had failed to look up these references until his work was completed, and then he found a large volume of morphological work which indicated that there were wide variations in the embryonic development of the three groups, and the poor fellow did not know to which group his original salamander belonged. That was a quarter of a century ago and as far as I am aware he has never been able to name his salamander or publish his results.

Most of you are familiar with the classical case of the entomologist who worked on the spermatogenesis of a certain species of insect or thought he did. He had the species in the wrong genus, worked up the wrong literature, found that it did not agree with the determinations made by European workers, wrote a strong criticism of their work only to have his material re-investigated and the discovery made that he had been wrong in his taxonomy and wrong in his morphology. Although not belonging to the genus, it *did* agree in the morphological changes.

There have been taxonomists who were equally indifferent to the biological and morphological relations of their work. All insects with long spines were placed in the group as against those with short spines. All dark insects were segregated from the light ones, entirely ignoring the fact that the length of spines or the color might easily be adaptations to certain food plants or environment and have occurred independently in groups of widely separated ancestry.

Evolution does not take place in structure alone or in function alone. Variations in animals take place in all lines, in structure, in function, in habit. It is only when we consider all of the factors in their relation to each other that we arrive at a true concept of the path of evolution.

The teaching of economic entomology has departed widely from that of the related sciences. The major portion of our textbooks has dealt with apple insects, corn insects, cotton insects, and the like. The student has a large amount of miscellaneous information of detailed life history and remedial measures centered around a certain crop plant and its environment. Instead he should obtain a thorough understanding of the fundamentals of insect biology so that if he meets a new pest he can apply his fundamental knowledge, and in a majority of cases have a fairly definite idea of the methods to use in control. Instead of getting the details of the 17-year locust in connection with the apple he may well learn that the *Cicadidae* as a group spend a long larval period in the earth, that their resemblance to an army tank is not accidental but an adaptation to that environment. He can then learn that the wireworms as a group also have a long larval period, that in general they have a definite relationship to weed growth or known cultivated crops, and even when he meets an exception to this general rule it will be noted as an exception only to emphasize the fundamental importance of the general adaptation. On the other hand, when he is studying the leaf-feeding forms he will readily realize that short larval periods are absolutely essential to the preservation of the species and will marvel at the many modifications which nature has worked out to adapt insects to the particular favorable period for this larval appearance. Such a course in entomology will train him to think and arouse his interest and enthusiasm, while the other course will be largely a training in memory and the mastery of definite details rather than the working out of principles and the development of theories.

In conclusion I would say that every entomologist should study taxonomy. In fact I would go further—that every entomologist should be a taxonomist in some group, large or small. If every economic worker would carry the responsibility for working out some small unit of our classification he would find it a wonderful stimulus to further development, as well as a broadening influence that would give him a wider series of contacts which would be of value. The aggregate of such small contributions would rapidly advance our knowledge of many little known groups, and if he selected his own economic group

for consideration it might easily change his whole viewpoint of the economic relations.

In the same way I believe every taxonomist should be deeply interested in and a student of the biology of his group, that as far as possible he should work with living material, and that in every case at least one or more species should be studied in large numbers, and thus develop the normal range of variation and adaptation within the species. In this way the systematist would be much clearer in his concept of what constitutes a species and be much more sympathetic with those who are struggling with biologic forms. In a number of fields it is becoming impossible to ignore the fact that there exist definite and fixed biologic forms which we can not, as yet at least, recognize by ordinary taxonomic characters.

Taxonomy as a whole has already reached a position where many divergent lines of proof can be brought to bear, all of which indicates that our major conclusions with reference to the evolution of our groups are accurate. A study of the parasites of the higher animals, for instance, shows a parallel development with that of the hosts. It shows that the parasites have differentiated as the hosts have differentiated. There are internal parasites and external ones; each one of these can be subdivided into different groups, and when the same evolutionary detail can be worked out for all of the groups each one will tend to confirm the accuracy and authenticity of the others. The writer was much interested a few years ago in checking up with Dr. W. D. Pierce on the classification of the Stylops in relation to the classification of the Jassidae and Fulgoridae that they parasitized. The Jassidae as a group are primitive with a certain number of specialized lines. The Fulgoridae as a group are highly specialized with only a few primitive lines. Dr. Pierce's classification of the Stylops indicated that the same relationship held with reference to the parasites. When taxonomy is approached from this standpoint it becomes one of the most valuable forms of biological study and can be recommended as part of the training of every entomologist and a part of the life work of a much larger number than at present.

PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

THE GEOLOGICAL SOCIETY

398TH MEETING

The 398th meeting was held at the Cosmos Club January 7, 1925, President STEPHENSON presiding. The Secretary announced the election to active membership of F. E. WHITE.

Program: Dr. LAUGE KOCH, Chief of the Danish Explorations, who spent six years in northern Greenland, addressed the Society on *The geology of Greenland: (1) Physiography and glaciology, (2) Structural geology and stratigraphy.*

399TH MEETING

The 399th meeting was held at the Cosmos Club January 28, 1925, President STEPHENSON presiding.

Program: Prof. FREDERICK J. PACK of the University of Utah addressed the Society on *Scenic aspects of Utah geology.*

HUGH D. MISER: *Erosion in San Juan Canyon, Utah.* The canyon of San Juan River extends west across a high arid region in southeastern Utah and joins the Glen Canyon of Colorado River near the southern boundary of the State. It reveals a magnificent geologic structure section possessing the same dimensions as the canyon, as much as half a mile high and 133 miles long. The rocks aggregate a thickness of 5000 feet and consist of limestone, sandstone, and shale, ranging in age from Pennsylvanian to Jurassic. Most of the rocks are red beds and, since soil is scanty and rock ledges abound, red is the predominating color in any landscape view. The rock strata have been flexed into a broad gentle arch, but neither the arch nor the minor structural features, such as anticlines, synclines, monoclines, faults and joints, have influenced the course of the river.

The present crooked course of the river in the canyon is a striking example of an entrenched meandering stream. Such a course may have been developed on a former cover of Tertiary sediments or on a peneplain, fragments of which stand near and above the walls. The peneplain is possibly of Pleistocene age, and the canyon cutting therefore apparently began in Pleistocene time. The cutting was rapid but did not continue uniformly as there were a few short pauses when the river was graded and deposited gravel which now floors benches of small area on the walls.

Rock debris, consisting of sand, gravel and boulders, forms the bed of the river and attains a depth of perhaps 100 feet or more. But it is presumably absent in a few of the rapids that are produced by inclined ledges of hard rock which cross the channel. Long stretches of the canyon, where the debris is deepest, present the peculiar example of an alluvial stream flowing between close walls of solid rock, but much of the debris is apparently moved by high floods that take place many years apart.

San Juan River carries an unusually large quantity of debris for streams in the United States and it is one of the chief contributors of mud to Colorado River. The water is always muddy, but during flood stages the river is actually a river of mud; and according to samples taken by Pierce it occasionally carries by volume three times as much silt as water. The heavy load of debris carried during floods causes a peculiar kind of waves known as sand waves.

These waves attain a height of about 7 feet and resemble those thrown up by a stern-wheel river steamboat. They travel upstream, in marked contrast to other kinds of waves that are stationery and also to waves that travel downstream.

If the proposed storage and power projects on San Juan and Colorado rivers are carried to completion the river, on reaching the heads of the reservoirs, will change its work from erosion to deposition. An important question concerning the reservoirs is, How soon will they be filled with rock debris? The answer to this question remains for the future, because the data available at present are not sufficient for making an estimate of the total load of debris that is carried each year by the San Juan and discharged into the Colorado. (*Author's abstract.*)

400TH MEETING

The 400th meeting was held in the Cosmos Club February 11, 1925, President STEPHENSON presiding.

Program: GEORGE P. MERRILL: *Early American geologists and their work.* (Illustrated with lantern slides.)

DAVID WHITE: *Geologic factors affecting and possibly controlling Pleistocene ice sheet development in North America.* A review of the physiographic and continental changes following middle Tertiary and Pliocene times can not fail to strengthen the idea that the movements of the Pleistocene ice sheets in North America, if not their origin itself, were determined mainly if not wholly by terrestrial factors. It appears more than possible that not only the regulation but the creation also of those ice caps will find adequate explanation in changes of level of the land, great reduction of the epicontinental seas, especially in the temperate and higher latitudes, the expansion of the continental surfaces, the corresponding differences in sub-oceanic topography, and the changes in ocean currents, air currents, rainfall and temperatures consequent to the changes in the land and water.

Among the conditions particularly to be taken into account are the emergence of the continents and the uplift of the higher land masses essentially to the maximum in the course of the post-Tertiary diastrophic revolution which probably is still in progress. Not only are the continental shelves in general unusually exposed, but the epicontinental seas, great stabilizers of continental climates, are in general greatly restricted. The Tertiary seas have largely receded.

The conclusion appears justified that the Laurentian shield embracing the Hudson Bay region was a broad plateau at the close of Tertiary time. Its drainage is an interesting and profitable study. No marine late Cretaceous, no marine Tertiary of any sort, lie on it in some ancient valley or flank it short of the Arctic coast, a great distance to the northwest. It was a land surface on which, in what is now the Arctic British-American Archipelago, fresh-water Tertiary basins existed during late Eocene or Miocene time, with climatic temperatures and rainfall favorable to the growth of temperate arboreal vegetation nearly to the 80th parallel. Within this region the so-called Arctic Miocene flora spread from Alaska through the Arctic islands, central Greenland, Spitzbergen, Nova Zembla, Franz Joseph Land and Siberia, plainly indicating a subsequent climatic revolution that could not take place without critical changes of land and sea. The post-Tertiary revolution lifted the northern lands much higher even than they are now. An elevation of 300 feet, which may well have taken place, would now close Bering Strait, and a

rise of 1200 or 1800 feet in the sea bottom around the north Atlantic would essentially connect up northeastern Europe and northwestern Asia through the islands very nearly to Greenland, leaving narrow and relatively shallow straits. Impressive geographical changes would follow an uplift of 750 feet. The mapping of the sea bottom in this region yields evidence in support of such changes and plant and animal distribution predicate recurrent uplifts or deformations sufficient to permit intercontinental migration of land animals and plants.

The geologic profession is too prone to close its notebook as soon as its feet are wet by tidal salt water. Not only its consecutive constructive thought, but its geological observations too often stop at tide level. The submerged beaches are not less interesting and significant than those so enthusiastically traced around the exposed lands; they may be as numerous and span even greater intervals. The topography and tectonics of the seas, soon to be traced by sonic sounding, are essential portions of the field of geology. The geologic history of the sub-oceanic regions is a subject for study inseparable from that of the land.

The northern Rocky Mountains have been shown to be progressive in growth. They were relatively low at the close of Tertiary time, so as to permit comparatively free transit of moisture-laden winds from the northern Pacific (possibly abnormally warm and moist if Bering Strait was temporarily closed) across to the elevated Laurentian plateau where, due either to post-Tertiary increase in elevation or changes in Arctic climate, the snow and ice of winter might have gained upon the melting capacity of summer's warmth, with consequent development and spread of the glacial ice sheet. Rise of the surface of the ice sheet itself naturally accelerated both the arrest of moisture and, through increased altitude, the lowering of mean temperature.

The strangulation of the north Atlantic and Bering Straits would conduce to great Arctic frigidity, with consequent marked effects on the climates of the northern lands.

Under loading of the ice sheet the shield should in due time have sunk isostatically, presumably with concomitant elevation to a minor extent of the land in portions of the peripheral zone, a procedure fairly well established for the Labrador sheet. Rasmussen reports shore terraces up to an elevation of over 1300 feet on Bylot Island and above 550 feet on Melville Island. Depression of the surface of the region to lower elevations would tend to raise the mean temperature, while retarded temporary elevation of the western border rim which, at maximum extension embraced the foothills, at least, of the Rocky Mountains, could only have cut off a portion of the moisture driven inward from the Pacific. Thus, conditions of reduced precipitation, especially in winter, and lowering of the land, should bring a check in the growth of the ice and reverse the annual increment of winter ice precipitation to annual gain of thaw, which, in due time, automatically would accelerate itself, with the lowering of the ice surface and the uncovering of the land mass. It may do no violence to facts yet observed to assume that the Hudson Bay region has been depressed and uplifted more than once since the development of the first ice sheet. Submerged beaches may alternate in time with some of the raised beaches between which the period of exposure of the land is so commonly distributed.

It is important, however, to note that the ultimate or concomitant mountain-building movement is marked by the final progressive uplift of the Rocky Mountain region, in the course of which morainal deposits on the flank of the mountains were raised to an elevation of around 4500 feet, as has

been noted by Alden. That the termination of Laurentian glaciation may have resulted both from the subsidence of Laurentian land itself on the one hand, and, on the other, the straining of moisture from the warm humid winds drifting from the north Pacific against the risen mountains, with consequent reduction of winter precipitation in the shield province, is indicated by the known elevation of the Rockies during this time, the evident depression of what is now the Archipelago region of British America, and the absence of all marine deposits of Mesozoic or Tertiary age in the entire Hudson Bay basin. This basin is to be viewed as now in the process of isostatic uplift, with slow emptying out of Hudson Bay. An elevation of 300 feet will drain the greater part of the Bay; 1200 feet will dry it. Irregularity of the movement, which was retarded as compared with the more prompt rebound of about 1000 feet in a portion of the Labrador-New England region, is geologically normal. The configuration of the basin, the strand deposits, and the presence of Pleistocene marine fossils at an elevation of 600 feet in the Hudson Bay depression show that the land has rebounded to a certain extent, though the Bay is not yet emptied and its floor is almost certainly many hundreds of feet below its maximum post-Tertiary elevation. Similarly, marine terraces have been noted by P. S. Smith along the north Atlantic coast and by Koch in northern Greenland at elevations of around 700 feet above tide. The argument predicates a continued rise of the Hudson Bay area but does not depend upon it. The great depression of this region is indicated also by the relatively recent (Pleistocene?) diversion of drainage in the western slope from north-south systems eastward into Hudson Bay.

In Eurasia changes of differential elevation and configuration of the land have received much attention, even on the part of biogeographers, but the possible consequent effects on the areas, depths, temperatures, densities and currents of the seas, and the joint effects on radiation, distribution of cyclonic centers and other climatic factors affecting the land do not seem to have engaged the close consideration that they deserve.

Obviously, outward movements of the continental shore lines, with closing (partial at least) of Arctic connections, which may tentatively be assumed to have occurred, and the assured existence of extensive ice sheets in the northern hemisphere could not fail, through their effects on water and air currents, to affect prevailing winds and weather in the subtropical and even the tropical areas, and, by disturbance of the equilibrium, in the Equatorial belt, with diversion of atmospheric currents, may well have affected the normal drift of warm air into the Antarctic zone. This is probable, regardless of the certain effects of post-Tertiary Andean uplift. If true it would favor the development of climatic cold and ice in the Antarctic regions contemporaneously, or nearly contemporaneously, with the growth of glacial sheets and extreme cold in the north. It will be recalled that Arctic mammals were driven into the lower Mississippi valley, while many indigenous plants and animals, including the horse, were exterminated.

The premises outlined above suggest factors possibly explanatory both of shifting in the areas of ice expansion and of some features of Cordilleran glaciation. The elevation of the interior region of North America in late Tertiary time, with the consequent expulsion of the sea from the Central region and even from the Mississippi embayment, taken in conjunction with the low stand of the Rocky Mountains at the earliest stage of the Pleistocene, would be favorable for the great extension of the earliest glaciation, Nebraskan and Kansan, to more southern limits in the Great Plains region. On the other hand, the growth of the Rocky Mountains from the south, with increas-

ing interference with transit of wind-borne humidity to that region, must have restrained the expansion of the later ice caps in that direction without corresponding effect on the spread of the northeastern sheets.

Similarly, the widespread glaciation of the Sierras while the Coast Ranges were relatively low contrasts strongly with the relatively restricted extent of the later glaciation at a time when the Coast Ranges were much more fully developed. Mountain growth to the west and consequent interference with precipitation on the Sierras may have terminated glaciation in this region, just as in the north, though, on the other hand, it is left to the glacialist and the climatologist to determine whether ice growth in the Sierra regions was not dominated by the causes and the growth itself of the great Laurentian sheets.

The author does not deny the possible effects of astronomical phenomena on earth climate. Variation in solar radiation, even for short periods like the three years of deficiency just past, may cause notable changes in ocean currents and air currents, with consequent marked effects on climate that may be felt in most unexpected quarters, once the approximate climatic equilibrium is disturbed, and by shifting and producing "highs" and "lows" of atmospheric pressure may touch off earthquakes, influence volcanic action, and if continued sufficiently long may cause isostatic adjustment. Pleistocene glaciation may, in his belief, largely if not wholly be explained by terrestrial rather than astronomical changes. At least the geologist should not look unto the heavens for help in the solution of his problem before he has duly and most earnestly considered all the facts already within his reach. The object of this presentation is to stimulate study of the questions here speculatively set forth. The more important evidence has to do with continental exposure, elevation, and configuration, Quaternary mountain building, elimination or reduction of epicontinental seas, migration of shore lines, strangulation of Arctic circulation, great changes in currents and temperatures of water and air, and changes in season and amount of precipitation.

The problem is one demanding the attention not only of the geologist, but of the oceanographer and the meteorologist, as well as the geographer. Its best solution can not be reached without their closest cooperation. When the causes of glaciation in Pleistocene time are determined it will be in order to consider the glaciations of earlier epochs. (*Author's abstract.*)

401ST MEETING

The 401st meeting was held in the Cosmos Club February 25, 1925, President STEPHENSON presiding. The Secretary announced the resignation from active membership of J. B. EBY.

Program: C. D. WALCOTT: *Robson Peak section.* (Illustrated with lantern slides.)

R. S. BASSLER: *The stratigraphic use of conodonts.* (Illustrated with lantern slides.) Certain Paleozoic formations particularly black shales, are often crowded with tooth-like fossils averaging a millimeter in diameter, resembling in part microscopic sharks' teeth. In 1856 a monograph upon Russian examples of these fossils was published by Pander who termed them "conodonts." Since then the systematic position of the conodonts has been much in question and little work of systematic value has been published upon them partly because it was believed that these structures were too variable to be of stratigraphic value. A detailed study of the ample collections of conodonts in the U. S. National Museum by E. O. Ulrich and the writer has

resulted in a classification of the group, which, if not entirely natural, has proved very useful in correlation. The work has also convinced us that the conodonts are the teeth of primitive fishes of perhaps several distinct groups and that the supposed great variability of structure in the same species does not exist. The conodonts show a marked evolution from simple undenticulated teeth in the Ordovician to complex forms with a main cusp and complicated denticulation in the Mississippian. Their value as horizon markers was proved particularly in working out the correlation of the Devonian and Mississippian black shales in the Eastern part of the United States, identical faunas having been found scattered over a wide range of country. (*Author's abstract.*)

WM. C. ALDEN: *Glaciation and physiography of Wind River Mountains, Wyoming.* Remnants of several finely developed sets of gravel-capped, cut terraces ranging from 15 or 30 feet to 1500 feet above the streams indicate successive notable stages of still-stand and stream planation alternating with stages of regional uplift. These have been described by Blackwelder, Westgate, and Branson and others. The Lenore terrace 15 to 30 feet above the streams is generally confined between the lines of bluffs. The moraines of the last, the Pinedale (or Wisconsin) stage of glaciation, extend down onto the Lenore terrace.

One to two hundred feet above the Lenore terrace is the Circle terrace. The moraines of the next older, or Bull Lake stage of glaciation, extend down onto but not below the Circle terrace. Some notable shifts in the locations of streams and valleys took place after the Bull Lake stage (which may correspond to the Iowan stage of the Keewatin ice sheet) and prior to the Pinedale stage. The Circle terrace seems to correspond to the main, or lower level of the second set of terraces throughout the Yellowstone drainage basin. There are scattered remnants of much more eroded higher and older sets of terraces some of which may correspond to the higher level of the second set of terraces on the Yellowstone of early Pleistocene age.

The highest tablelands 1000 to 1500 feet above the streams, represented by the top of Table Mountain near Lander, are remnants of a vast gravelly alluvial piedmont terrace (the Table Mountain plain). This is believed to have been completed in Pliocene time and to be the correlative of Meeteetse terrace in Big Horn Basin and of the Flaxville and associated terraces of Montana. The much-weathered deposits of big boulders capping Table Mountain and other remnants are believed (in part at least) to have been deposited by mountain glaciers on the Table Mountain piedmont terrace before it was much dissected. This is probably as old as, or older than, Blackwelder's "Buffalo drift" of the Wind River and Teton Mountain region. It is probably the correlative of similar deposits of early Pleistocene age on high mesas at the east front of Big Horn Mountains, and of the oldest mountain glacier drift (Nebraskan) on the highest benchlands in the region of Glacier National Park.

Several thousand feet above the Table Mountain remnants is the summit peneplain or plateau on Wind River Mountains, the product of Blackwelder's Fremont cycle of erosion. In the opinion of the writer this is older than Pliocene, possibly Miocene or Oligocene. Notable regional uplifts followed both the development of this peneplain and that of the Table Mountain plain. The latter uplift probably closed the Tertiary, brought on the first mountain glaciation, and started the streams to dissecting the Table Mountain plain or piedmont terrace. These correlations are, as yet, tentative. (*Author's abstract.*)

402D MEETING

The 402d meeting was held in the Cosmos Club March 11, 1925, President STEPHENSON presiding. The Secretary announced the election to active membership of R. L. FARIS.

Program: E. T. ALLEN: *Further evidence of the nature of hot springs.* (Illustrated by lantern slides.) Drilling for natural steam as a source of power has been in progress for some time at a place called "The Geysers," Sonoma County, California. "The Geysers" are situated in the St. Helena Mt. Range. The hot areas in this locality extend along the side of a narrow canyon, occurring at intervals for a distance of about six miles. No igneous rocks, lava or volcanic ejecta have been discovered in the immediate neighborhood; the rocks are sandstones, shales, serpentines, schists, and other metamorphics. At The Geysers sandstone is encountered by drilling at a depth often less than 100 feet from the surface. The temperature close to the surface is very generally near 100°C. As cracks are cut by the drill the steam flow increases and the temperature rises rapidly—25°C. or more per 100 ft. in the upper strata, and measurements show that water could not penetrate to any considerable depth without being vaporized. Small hot springs often of high mineral concentration are frequent. Their maximum temperature reaches about 98°C.—the boiling point of water for the elevation. The mineral matter in the springs is chiefly sulphate and acid sulphate of ammonium and magnesium, or in the alkaline springs carbonate, bicarbonate and sulphate. The evidence shows that the volatile matter is derived from the volcanic gases which are escaping from springs and fumaroles. The non-volatile matter is derived from the serpentine and other metamorphic rocks and of the area. That it comes from rocks near the surface is supported by the fact that surface water can not penetrate deeply and fresh pyrite in the sandstone drillings shows that oxidation also extends only to shallow depths.

The phenomena of The Geysers are best accounted for on the assumption that superheated steam and other volcanic gases are ascending from a hot batholith through a deep crack in the overlying strata; that the steam is heating surface water by condensation, and the gases, hydrogen sulphide and carbon dioxide through logical chemical changes are decomposing the superficial rocks. (*Author's abstract.*)

W. H. BRADLEY: *An interpretation of the Green River formation.* (Illustrated by lantern slides.) The field observations on the Green River formation and study of the microfossils of the oil shale together with the application of the principles of limnology to the interpretation of these lake beds indicate the following trend in the formation's geologic history.

The Green River lakes were initiated, by gentle downwarping of the basin floors, as large and relatively stable though quite shallow fresh water lakes in which flourished an abundant flora and an active fauna. Limy shale, oolitic limestone, sandstone, and a small amount of low grade oil shale were deposited in the lakes of this stage.

The lakes of the second stage were still shallower and under climatic influence repeatedly filled and evaporated either partially or completely. At the beginning of the cycle they may be pictured as broad sheets of clear and fresh or moderately alkaline water, but at the close of the cycle, after a long, hot, dry season, as a large number of disconnected ponds of various sizes and various degrees of alkalinity. Plankton organisms, mostly algae and Protozoa, thrived in these ephemeral ponds and reduction in volume not only greatly

concentrated those already present, but also stimulated a vastly greater production of them by reason of the stagnation and corresponding rise in temperatures of the water. Active putrefaction, probably also assisted by the activity of saprophytic fungi, protozoans, various round worms, and minute crustaceans reduced the dead organisms to a nearly structureless jelly. Organic acids resulting from the putrefaction together with the increasing content of dissolved mineral salts finally became effective toxins and the ooze became an almost perfectly antiseptic medium. This structureless, semifluid organic ooze with its occluded algae, fungi, protozoa, pollen, and spores together with various proportions of finely divided mineral matter was then covered by the deposits of the next cycle and subsequently lithified into oil shale.

The closing phase of Green River deposition was characterized by strongly alkaline lakes, probably playa-like, in the wet muds of which considerable glauberite crystallized out. The conditions of deposition of this third phase were evidently too unfavorable for the growth and accumulation of large quantities of microorganisms and only an insignificant amount of oil shale resulted. A period of vigorous stream abrasion terminated the alkaline playa phase and was followed by the deposition of stream channel sands and fluviatile clay which now make up the top of the Green River formation. (*Author's abstract.*)

KIRK BRYAN: *Recent deposits of Chaco Canyon, New Mexico, in relation to the life of the pre-historic peoples of Pueblo Bonito.* (Illustrated by lantern slides.) The part of the valley of Chaco River known as Chaco canyon lies on the southern border of San Juan Basin, New Mexico, and is about 12 miles long, from 1 to 3 miles wide, and 200 to 400 feet deep. The ruins of 13 large communal houses stand on the canyon floor and the adjacent cliffs and with numerous smaller ruins testify to the flourishing civilization that once existed in this now desolate region. Pueblo Bonito, the largest of these ruins, is now being excavated by an expedition under Neil M. Judd, organized and supported by the National Geographic Society. The geologic work here recorded in a preliminary statement was done at the instance of Mr. Judd and at the expense of the Society.

The flat floor of Chaco Canyon is marked by a relatively recent gully in which the floods of Chaco River are now wholly confined. This gully, or arroyo, is now fron 150 to 450 feet wide and 30 feet deep at Pueblo Bonito. In 1877 it was 40 to 60 feet wide and 16 feet deep. An exploring expedition in 1849 makes no mention of an arroyo and it therefore seems likely that the arroyo was initiated at about the same time as the similar gullies in other southwestern canyons, i.e., since the year 1870.

The alluvial deposits that form the floor of the canyon are of unknown thickness, but the upper 30 feet is well exposed in the walls of the arroyo. The sand, silt, "adobe," and gravel of this valley fill are all characteristic of deposition in muddy floods similar to those of the present ephemeral stream and give no evidence that the Canyon ever had a perennial water course. The agriculture of the prehistoric peoples was, therefore, not carried on by irrigation with living water but probably by the methods of floodwater farming—a system still in use in the region.

Relics of man, in the form of hearths, bone fragments, potsherds, etc., are found to a depth of 21 feet. The zone from 4 to 6 feet below the surface contains the remains of the people who built the large ruins and was therefore deposited in the Pueblo period. The zone from 6 to 21 feet contains the relics

of pre-Pueblo peoples. In addition to this normal relation showing a transition in the type of human culture with the progress of alluviation of the valley, there is a buried channel containing potsherds of the latest type known in Pueblo Bonito. The buried channel is from 15 to 18 feet deep and has been traced a total distance of 1500 feet. It was evidently formed and refilled either very late in the occupancy of Pueblo Bonito or shortly after its abandonment, and represents a post-Bonito or post-Pueblo period.

If this buried channel represents a period of erosion followed by a period of sedimentation intervening between the period of alluviation that formed the main valley fill and the present period of erosion which began in 1870, very important consequences result. The formation of the channel would have so reduced the agricultural area subject to floodwater farming as to furnish an approximate cause for the abandonment of Pueblo Bonito. The refill of the channel would have restored the flood plain to a condition nearly like its original condition and thus would have provided conditions suitable for the expansion of the Navajo tribe in the years before and since the Spanish conquest.

Further investigation is planned for the purpose of tracing this buried channel and further unravelling the history of the valley fill. Since, however, the various members of the complex mass of otherwise similar sediments contain potsherds of characteristic type, the problem can be attacked by ordinary stratigraphic methods in which potsherds take the place of fossils. (*Author's abstract.*)

403D MEETING

The 403d meeting was held in the Cosmos Club March 25, 1925, Vice-President HEWETT presiding.

Program: W. T. SCHALLER: *Genesis of lithium pegmatites.* (Illustrated with lantern slides.) Studies in the field and laboratory of the lithium pegmatites of southern California and laboratory studies of similar specimens from other localities have shown that these pegmatites as now composed are not original crystallizations from a magma but are a hydrothermal replacements of a much simpler, earlier formed, magmatic rock free from any lithium minerals. In the California field graphic granite was the earlier rock replaced, the well-defined texture of graphic granite serving as a "key" for the determination of the replacement processes. In volume percentage, albite is the chief mineral replacing both the microcline and quartz of the graphic granite. Other stages of the replacement show that the albite was later replaced by lithium minerals. The well known rubellite and lepidolite specimens were shown to have been originally graphic granite. Other pegmatitic minerals accompanying the albitic and lithium mineralic replacements include muscovite, biotite, garnet, black tourmaline, beryl, columbite, etc. The original pegmatitic magma was therefore not rich in the so-called mineralizers, all of which were introduced later in the hydrothermal replacement processes. The formation of some of these minerals in bands and the formation of crystal lined cavities are likewise due to the replacement processes. (*Author's abstract.*)

FRANK L. HESS: *Oölites.* (Illustrated with lantern slides.) Some oölites from Carlsbad Caverns, New Mexico, were turned over to the speaker for study. They were formed through the precipitation of calcium carbonate in small pools under stalactites, the drip from which stirred the water of the pools,

so that precipitating lime carbonate was kept in motion. It was shown by other examples, including calcium carbonate oölites, formed in boiling sugar refuse; nickel oölites formed from gas in the regular production of nickel by the Mond carbonyl process; sulphur oölites formed in crater lakes from sulphurous gases bubbling through the water; hailstones apparently formed through the precipitation of ice from gaseous H_2O; and oölites formed in Great Salt Lake from calcium carbonate brought down by streams, that the same principle governed the formation of all these different types of oölites, that is, that many solids precipitated in a moving fluid take on an oölitic form, and it is therefore unnecessary to call on the aid of bacteria or other more or less mysterious agencies to explain the formation of oölites. Calcite, hematite, phosphorite and oölites have apparently all been formed in the same way. (*Author's abstract.*)

W. P. Woodring: *Miocene climate of tropical America.* (Illustrated with lantern slides.) At the close of Miocene time many genera of marine animals suddenly disappeared in the Caribbean Sea and adjoining waters of the Gulf of Mexico and Atlantic Ocean. Most of these genera that are now living are found in the warm waters of the eastern Pacific and some are confined to the tropical western Pacific. Therefore it seems probable that in its physical features the Miocene Caribbean Sea resembled the present tropical Pacific. Among the mollusks characteristically tropical families such as the Terebridae, Conidae, Cancellariidae, Mitridae, Columbellidae, Cypraeidae and Arcidae had a much richer representation in the Miocene Caribbean Sea than in the present Caribbean Sea. The whole Miocene Caribbean fauna had a more tropical aspect than the living fauna.

Perhaps the most significant feature accounting for the change in the Caribbean Sea and the extinction there of Pacific genera lies in the closing of the channels that extended across Panama and Costa Rica during Miocene and earlier Tertiary time. The problem of the effect of these channels on oceanic circulation can hardly be avoided even though available data may be too meager to attempt to evaluate its significance. All geologists who are familiar with Central America agree that the channels were open during at least parts of Eocene, Oligocene, and Miocene time and were closed after middle Miocene time. Some geologists believe that water from the Caribbean Sea was transferred into the Pacific across the Central American channels. It seems more probable that the slightly higher mean sea level in the Pacific and the much greater tidal range on the Pacific side of Central America would cause a movement in the opposite direction, carrying into the Caribbean Sea water having the same temperature, salinity, and food supply as Pacific water, and offering a means of transporting pelagic larvae. As the Tertiary faunas of the Pacific coast of Central America and northern South America are being studied, it is becoming apparent that, at least so far as the mollusks are concerned, many of the Pacific genera that have disappeared in the Caribbean Sea were autochthonous in the eastern Pacific. They moved into the Caribbean Sea as temporary migrants and remained there only as long as the channels were open. This explanation is far from satisfactory when an attempt is made to apply it to genera that were established in the Caribbean Sea during the long period from Eocene to Miocene and even spread as far north as the southeast coast of the United States. It is purely speculative, but it seems to offer a reasonable basis to account for the striking change in the Caribbean fauna—a change caused by the elimination or impoverishment of genera and families that are characteristically tropical. (*Author's abstract.*)

404TH MEETING

The 404th meeting was held in the Cosmos Club April 8, 1925, Vice-president BUTTS presiding.

Program: J. B. MERTIE, JR.: *The Paleozoic geology of interior Alaska.* The oldest rocks in Alaska comprise a group of rocks, known collectively as the Birch Creek Schist, which crop out typically in the valley of Tanana River, in interior Alaska. These consist essentially of quartz-mica schist of sedimentary origin, together with orthogneiss and metamorphosed basic igneous rocks. The schist is of pre-Ordovician and probably of pre-Cambrian age. The included metamorphic igneous rocks may be in part of early Paleozoic age.

Overlying unconformably the Birch Creek Schist in its type locality is a great thickness of slate and metamorphosed arkose, known as the Tatalina group, in the upper part of which occur lower Ordovician fossils; and at the very top of the Tatalina group is found a formation of metamorphosed basaltic lavas, in the tuffs of which late middle Ordovician fossils occur. To the eastward, along the international boundary, rocks which are believed to be stratigraphically equivalent to the Tatalina group contain fossils ranging in age from middle Cambrian to upper Ordovician; while to the southward and southwestward, both Utica and Richmond horizons in the upper Ordovician have been recognized, the former being essentially an argillaceous and the latter a limestone formation.

Lower Silurian rocks have not been found in the interior of Alaska, but middle Silurian rocks are widespread and form one of the best known horizon markers in the Paleozoic section. The type locality is in Brooks Range, of northern Alaska, where a middle Silurian limestone about 6000 feet thick, extends for more than 500 miles from east to west across the Territory. A similar limestone of like age is found in the Yukon-Tanana region, and along the international boundary. Upper Silurian time is represented in northern Alaska by a formation of slate, quartzite and relatively thin bands of limestone; and rocks of similar character and age, in the Yukon-Tanana region are grouped together under the designation Tonzona group.

A great structural inconformity is believed to separate the Silurian rocks from the overlying Devonian rocks; and during this stratigraphic hiatus in lower Devonian time, granitic rocks, now gneissoid, are believed to have originated in Northern Alaska, and perhaps in the Yukon-Tanana region. The succeeding middle and upper Devonian rocks, and the base of the Mississippian sequence of northern Alaska, are essentially quartzitic sandstone and slate, with some limestone beds. These rocks are strikingly free from igneous intrusives and extrusives but south of Brooks Range, and particularly in the Yukon-Tanana region, ancient basic lavas are interbedded with middle Devonian limestones, and ultra-basic intrusives, believed to be of upper Devonian age, are found. Similarly, in the Yukon basin, another great series of basic lavas and intrusives, known as the Rampart group, are found, which contain Mississippian fossils in their tuffaceous members.

The Carboniferous rocks of northern Alaska include the basal sandstones and shales, known as the Noatak formation; an overlying formation of limestone and chert, known as the Lisburne formation; and possibly a still higher chert formation, all three being of Mississippian age. The Lisburne formation, like the Silurian limestone, is a well defined horizon marker, extending 600 miles east-west across the Territory. The Pennsylvanian is represented by the Sadlerochit sandstone, but in northern Alaska, Permian rocks have

not been found. Southward, in the Yukon basin, several formations of Carboniferous rocks are known. The Mississippian rocks include the Rampart volcanics, the Calico Bluff formation composed of thin-bedded shale and limestone, the Livengood Chert formation, and a chert-argillite (?) group of rocks. These formations occur at different localities, and the stratigraphic relations between them are therefore obscure. The Pennsylvanian is believed to be represented by the Nation River formation, which may be in part of nonmarine origin; and the Permian is represented by a marine highly fossiliferous limestone.

A regional uplift of all of Alaska began in late Carboniferous time, culminating perhaps in the lower Triassic, with the resumption of marine sedimentation in the upper Triassic. This elevation was of the continental type and was accompanied by a minimum of rock deformation and mountain building, though it doubtless resulted in tilting and some folding of the rocks. In Jurassic time, however, another great elevation of interior Alaska occurred, accompanied by orogenic movements of the first magnitude, and the intrusion of great granitic batholiths. It was during this, and subsequent periods of similar tangential thrusting from the south, in Cretaceous and Tertiary time, that the late Paleozoic rocks of interior Alaska acquired their present complex structure. (*Author's abstract.*)

GEORGE C. MARTIN: *The Mesozoic rocks of Alaska.*

PHILIP S. SMITH: *Fields for future Alaskan studies.* (Illustrated with lantern slides.) The speaker exhibited a map of Alaska on which were distinguished the areas that have or have not been surveyed by the U. S. Geological Survey in the territory. He discussed the physical conditionof the various unsurveyed tracts and the major geologic problems of each and pointed out those areas which because of their promise of containing mineral deposits of value were most worthy of early investigation. Large tracts of the lowlands adjacent to the coast which because of their physical condition afford little information to the geologist regarding the unusual resources and which because of their swamps will be extremely expensive to map, will probably not be surveyed until airplanes or some other effective means of transportation are available. (*Author's abstract.*)

EDWARD SAMPSON, J. D. SEARS, *Secretaries.*

SCIENTIFIC NOTES AND NEWS

Dr. E. W. WASHBURN has been appointed Chief of the Chemistry Division, Bureau of Standards to succeed Dr. W. F. HILLEBRAND, whose death occurred about a year ago. Dr. WASHBURN is a graduate of the Massachusetts Institute of Technology with the degrees of B.S. and Ph.D. He was on the staff of the University of Illinois from 1908 to 1922 as Professor of Physical Chemistry and as Head of the Department of Ceramic Engineering. For the past three years he has served as Editor-in-Chief of the International Critical Tables. His principal research work has been on the hydration of ions, theory of solutions, and the high temperature chemistry and physics of ceramic materials.

Dr. SEWALL WRIGHT, formerly of the U. S. Department of Agriculture, has been appointed associate professor of zoology at the University of Chicago.

The Ore Deposits Club met on Friday evening, January 22, at the Geological Survey. Messrs. LOCKE and KINGSBURY discussed *Mineralization stoping.*

A circular has been issued by Chairman DAVID WHITE on behalf of the Division of Geology and Geography of the National Research Council, in cooperation with the Geological Society of America, outlining plans for the Fourteenth Geologic Congress which will be held at Madrid, Spain, May 24 to 31, with excursions preceding and following these dates. A number of Washington geologists are planning to attend the Congress.

Obituary

Dr. WILLIAM EDWIN SAFFORD, a member of the ACADEMY and a frequent contributor to its Journal, died of pneumonia on January 10, in his 67th year. After more than twenty years' service in the United States Navy, during which he acted for one year (1899–1900) as vice-governor of Guam. Dr. SAFFORD entered the Department of Agriculture in 1902 and was at the time of his death economic botanist of the Bureau of Plant Industry. Dr. SAFFORD was a man of unusually broad interests and pronounced linguistic and literary ability, whose numerous publications on economic plants were enriched by his knowledge of ethnology, art, and languages. During his service in the Department of Agriculture he devoted himself particularly to the study of the plants and plant products used by the American aborigines, especially of the tropics, and of the early history of the cultivated plants both of the Old and the New Worlds. At the time of his death he was bringing to completion a work on the useful plants of Mexico. His principal botanical publications include a flora of the island of Guam and revisionary works dealing with the Annonaceae and the bullhorn acacias.